in one way or another, the cavities of the water-conducting elements, and then observing whether the current is interrupted. Sachs and Dufour endeavoured to attain this result by sharply bending the stems of actively-transpiring plants, but this method is obviously unsatisfactory, owing to the difficulty of proving that the cavities are completely closed. Elfving attacked the problem in a different way. He injected portions of the stem of woody plants with cocoa-butter, melted at a temperature of 30° C., and satisfied himself that the cavities were really filled up when the injected material had solidified. Under these conditions he found that a pressure of 60 cm. of mercury failed to force any water through the wood, though before the injection 1 cm. of water had sufficed to cause filtration.

To Elfving's experiment two objections have been made. On the one hand, Dufour urged that the absence of the action of transpiration, rather than the closure of the cavities, might well explain the result of the experiment. On the other hand, it was objected by Scheit that the action of the fatty cocoa-butter on the membranes would probably render them impermeable to water, and thus account for a negative result. Prof. Errera has succeeded in modifying Elfving's method in such a way as to meet both these objections.

In the first place, actively transpiring branches were employed for the investigation, *Vitis vulpina* being selected for experiment on account of the large diameter of its vessels. Secondly, instead of cocoa-butter, a solution of gelatine melting at 33° C. was used as the injecting material. This was coloured with Indian ink, so that its presence in the vessels might be easily demonstrated. The action of transpiration was in most cases assisted by the pressure of a column of water 50 cm. in height. The experiments were carried out with all possible precautions, and the result in every case was that the injected branches took up no water, and faded in a few hours, while, under precisely similar conditions, uninjected branches remained perfectly fresh for three days at least, and during that time transpired many cubic centimetres of water. For details and numerical results we must refer to the original.

Prof. Errera's experiments certainly add greatly to the already strong probability that the cavities of the tracheal elements of the wood constitute the channels through which the sap ascends. 　　　　　　　　D. H. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*
[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Lost Found—Boole Justified and Monge Reinstated in His Rights by Prof. Beman of the University of Michigan, U.S.

IN the report of my public lecture on Reciprocants, published in NATURE of January 7 (p. 222), mention is made of a formula, given by Boole in his book on "Differential Equations," which he ascribes to Monge.

Endeavours were instituted in London, Cambridge, and Paris to ferret out the passage in Monge in which it occurs, and very diligent search was made, as well in the printed works as in the manuscripts of Monge in the library of the Institute, to accomplish this object.

But all these researches were fruitless, and the opinion was come to by the compatriots of Monge that Boole had made a misquotation, and that the formula ascribed by him to Monge was not to be found in his works. The formula is one of very great interest, as being the first instance on record of a multinomial projective reciprocant.

Knowing how scrupulous and painstaking Boole was, and the

least likely of all men to make a quotation at random, I never acquiesced in this belief, but entertained little or no hope that any one would ever succeed in unearthing a reference which had defied all the endeavours of Monge's own countrymen to verify.

But fate had designed otherwise, as will be seen from the subjoined letter. In addition to the satisfaction of a controverted point being settled and Boole's character freed from a rash imputation of inaccuracy, it is to my mind, and will probably be so to many of the readers of NATURE, a peculiar source of pleasure to contemplate the occurrence as an illustration or note of the unity not merely of occupation, but of feeling also, which binds together mathematical workers in all parts of the world.

To think that a task found impossible in London and Paris should have been accomplished in the most satisfactory manner at Yale and Michigan !

Without further comment I submit the letter in its entirety as written, for the insertion which it so well merits in the world-wide-diffused columns of NATURE, and think that all its readers will join with me in according a cordial vote of thanks to Prof. Beman for his valuable contribution to mathematical history.

University of Michigan, Ann Arbor, Michigan, April 3, 1886

PROF. J. J. SYLVESTER, Oxford, England

DEAR SIR,—You will find Monge's form of the differential equation of the conic in his memoir, "Sur les Équations différentielles des Courbes du Second Degré" (Corresp. sur l'École Polytech. Paris, ii., 1809-13, pp. 51 54), and in *Bulletin de la Soc. Philom.*, Paris, 1810, pp. 87, 88 ; the first as having been contributed directly by Monge, and the second as having been copied from the first.

I have not *seen* the journals myself, but the references have been *verified* for me at the Yale College Library. The actual form is "$9q^2t - 45qrs + 40r^3 = 0.$"

The term "Mongian" can now be used without hesitancy by you.

I remember noticing this form when I began reading Boole's "Differential Equations," and I also noticed Halphen's method in Jordan's "Cours d'Analyse." It never occurred to me that Halphen considered the *form* original with himself ; I thought that his *method*, probably, of deducing it was different from Monge's.

With kind recollections of having met you at Johns Hopkins once upon a brief visit when Prof. Cayley was there,

I am yours very sincerely,
W. W. BEMAN,
Assoc. Prof. Math.

Since writing the above, in fact this very afternoon, I have received a letter from the Universal Knowledge and Information Office containing the same references as those given by Prof. Beman, which will speak for itself, and cannot fail to draw the attention of the readers of NATURE to the important service which this Society is capable of rendering to all engaged in research of whatever nature in enabling them to discover the origins and hunt up the authorities of any subject on which they may desire to obtain information.

It is certainly a singular coincidence that after the lapse of four months the desired information in this case should have reached me from such widely distant sources at an interval of less than forty-eight hours. The letter, which I inclose, is well deserving of setting out in full. The reference made to the circle at the end is extremely interesting, as it contains an example of a non-homogeneous mixed reciprocant, which in the notation now in use might be written $(1 + t^2)b - 3ta^2$. Or rather, adopting the improved notation, in which $t, a, b, \ldots$ represent

$$\frac{dy}{dx}, \quad \frac{1}{1.2}\frac{d^2y}{dx^2}, \quad \frac{1}{1.2.3}\frac{d^3y}{dx^3}, \ldots$$

it takes the form

$$(1 + t^2)b - 2ta^2.$$

London, April 15, 1886

DEAR SIR,—I am instructed by the management to send you the following in reference to your question relating to the attribution of the differential equation

$$\left(9\left(\frac{dy}{dx^2}\right)^2 - 45\frac{d^2y}{dx^2}\cdot\frac{d^3y}{dx^3}\cdot\frac{d^4y}{dx^4} + 40\left(\frac{d^3y}{dx^3}\right)^3 = 0\right)$$

to Monge by Boole in his "Differential Equations."

In the *Nouveau Bulletin des Sciences, par la Société Philo-mathique de Paris*, tome ii., Paris, 1810, occurs this passage :—

"Mathématiques.—Sur les Équations différentielles des Courbes du Second Degré, par M. Monge. L'équation générale des courbes du second degré étant

$$Ay^2 + 2Bxy + Cx^2 + 2Dy + Ex + 1 = 0,$$

dans laquelle *A, B, C, D, E* sont des constantes, M. Monge donne l'équation différentielle débarrassée de toutes ces constantes, et il parvient à l'équation suivante, du cinquième ordre,

(A) $$9q^2t - 45qrs + 40r^3 = 0,$$

les quantités *r, s, t,* étant définies par les équations suivantes :

$$\frac{dy}{dx} = p, \quad \frac{dp}{dx} = q, \quad \frac{dq}{dx} = r, \quad \frac{dr}{dx} = s, \quad \frac{ds}{dx} = t.$$

"Il faut ensuite voir l'usage de l'équation (A), pour trouver l'intégrale d'une équation d'un ordre inférieur qui satisfait à cette équation (A) ; ainsi étant donnée l'équation différentielle $(1 + p^2)r = 3pq^2$, il parvient à l'intégrale $(x - a)^2 + (y - b)^2 = c^2$, qui est l'équation d'un cercle.

"La même méthode pourroit s'appliquer aux équations des courbes d'un degré supérieur au second."

A note is added to the ,effect that "Cet article est extrait de la Correspondance de l'École impériale Polytechnique, rédigée par M. Hachette : 1r cahier du 2e volume, 1810." The press mark of this work at the British Museum is PP. 1543.

Trusting that this is the reference you are in search of, and that the long delay in the discovery of it may be excused when the difficulty of identifying a particular passage (known perhaps only in its full extent to those whose chief work is concerned with such matters) is considered.

I remain, Sir, faithfully yours,
H. FISHER

PROF. J. J. SYLVESTER, &c., &c.

New College, Oxford, April 19   J. J. SYLVESTER

On the Velocity of Light as Determined by Foucault's Revolving Mirror

IT has been shown by Lord Rayleigh and others that the velocity (U) with which a group of waves is propagated in any medium may be calculated by the formula—

$$U = V \left(1 - \frac{d \log V}{d \log \lambda} \right),$$

where V is the wave-velocity, and λ the wave-length. It has also been observed by Lord Rayleigh that the fronts of the waves reflected by the revolving mirror in Foucault's experiment are inclined one to another, and in consequence must rotate with an angular velocity—

$$\frac{dV}{d\lambda} \alpha,$$

where α is the angle between two successive wave-planes of similar phase. When $dV/d\lambda$ is positive (the usual case), the direction of rotation is such that the following wave-plane rotates towards the position of the preceding (see NATURE, vol. xxv. p. 52).

But I am not aware that attention has been called to the important fact, that while the individual wave rotates the wave-normal of the group remains unchanged, or, in other words, that if we fix our attention on a point moving with the group, therefore with the velocity U, the successive wave-planes, as they pass through that point, have all the same orientation. This follows immediately from the two formulæ quoted above. For the interval of time between the arrival of two successive wave-planes of similar phase at the moving point is evidently $\lambda/(V - U)$, which reduces by the first formula to $d\lambda/dV$. In this time the second of the wave-planes, having the angular velocity $\alpha dV/d\lambda$, will rotate through an angle α towards the position of the first wave-plane. But α is the angle between the two planes. The second plane, therefore, in passing the moving point, will have exactly the same orientation which the first had. To get a picture of the phenomenon, we may imagine that we are able to see a few inches of the top of a moving carriage-wheel. The individual spokes rotate, while the group maintains a vertical direction.

This consideration greatly simplifies the theory of Foucault's experiment, and makes it evident, I think, that the results of all such experiments depend upon the value of U, and not upon that of V.

The discussion of the experiment by following a single wave, and taking account of its rotation, is a complicated process, and one in which it is very easy to leave out of account some of the elements of the problem. The principal objection to it, however, is its unreality. If the dispersion is considerable, no wave which leaves the revolving mirror will return to it. The individual disappears, only the group has permanence. Prof. Schuster, in his communication of March 11 (p. 439), has nevertheless obtained by this method, as the quantity determined by "the experiments hitherto performed," $V^2/(2V - U)$, which, as he observes, is nearly equal to U. He would, I think, have obtained U precisely, if for the angle between two successive wave-planes of similar phase, instead of $2w\lambda/V$, he had used the more exact value $2w\lambda/U$.

By the kindness of Prof. Michelson, I am informed with respect to his recent experiments on the velocity of light in bisulphide of carbon that he would be inclined to place the maximum brilliancy of the light between the spectral lines D and E, but nearer to D. If we take the mean between D and E, we have—

$$\frac{K}{U} = 1 \cdot 745, \qquad \frac{K(2V - U)}{V^2} = 1 \cdot 737,$$

K denoting the velocity *in vacuo* (see *Amer. Jour. Sci.*, vol. xxxi. p. 64). The number observed was $1 \cdot 76$, "with an uncertainty of two units in the second place of decimals." This agrees best with the first formula. The same would be true if we used values nearer to the line D.

J. WILLARD GIBBS

New Haven, Connecticut, April 1

The Effect of Change of Temperature on the Velocity of Sound in Iron

I VENTURE to draw attention to an error relating to the above subject, which, originating with Wertheim, still holds a place in some of our modern books on science. According to Wertheim, the velocity of sound in iron and steel is *increased* by rise of temperature not extending beyond 100° C. Now in no sense whatever is this statement correct. It is true that the longitudinal elasticity of iron, as determined by the static method. will be found greater at 100° C. than at 0° C., provided *we begin with the lower temperature first and the wire has not, after the original annealing, been previously raised to* 100° C. ; but the apparent *temporary* increase of elasticity is really a *permanent* one (*Phil. Trans.*, part i., 1883, pp. 129–131), and if the wire be repeatedly heated to 100° C. and afterwards cooled, subsequent tests will always show a *less* elasticity at the *higher* temperature than at the lower, if *sufficient rest after cooling be allowed.* When, however, we come to such small molecular displacements as are involved in the passage of sound through a wire, even the apparent increase of elasticity mentioned above vanishes. I have been able to prove that, when an iron or steel wire is thrown into longitudinal vibrations, so as to produce a musical note, the pitch of this note becomes *lower* as we *raise* the temperature, *even when the wire is heated for the first time* after it has left the maker's hands.

It seems rather strange that this error should have so long been allowed to remain uncorrected, for it has been known for many years that the pitch of a tuning-fork made of steel is lowered by small rises of temperature, and the main part of this lowering must be due to the *decrease* of elasticity of the steel.

HERBERT TOMLINSON

King's College, Strand, April 10

Sound-producing Apparatus of the Cicadas

WITH regard to the above subject, treated of in an article by Mr. Lloyd Morgan in February last (NATURE, February 18, p. 368), I may mention that some time ago I examined the drum of the common cicadas found plentifully in the Himalaya near Simla, and which in the evenings send forth a deafening roar from the rhododendron-trees like the whirr of large machinery. Generally the arrangement of the drum and the powerful muscles was as figured by Mr. Morgan, but I also noticed the following particulars not mentioned by him.

The chitinous rods in the membrane of the drum were not parallel, but converged slightly towards one point of the mem-